

vessels arose from the arch; the arteria innominata passed to the left side; the vena cava ascendens was on the left side of the spine.

In conformity with the altered position of the large trunks, the distribution of the various branches passing to and from the viscera was changed relatively; the recurrent branch of the pneumogastric nerve was given off on the right side round the arch of the aorta, on the left round the subclavian artery. In fine, the most perfect adaptation was observed throughout.

## PHYSIOLOGY.

3. *On the Causes of the Motion of the Blood in the Capillary Vessels.* By Dr. POISEUILLE.—When the globules of the blood in the capillaries of the mammifera are examined, they are found to possess different velocities, even in the same vessel. Some of them have two simultaneous motions—one of rotation, the other of translation; while others remain motionless for a time. Two globules, presenting at first the same velocity, only preserve by accident the distance which separates them, and, if the motion be such as to permit us to follow the same globule, we can observe it sometimes in the same capillary vessel presenting these different phases of motion. The velocity of the globules in the capillaries is less than in the arteries and veins; it is seldom greater. This remark extends also to a capillary vessel which rises immediately from an artery, or which proceeds directly into a venous trunk. These different phenomena lead to the conclusion that the globules are endued with a spontaneous motion, or, rather, that the cause of the flow of blood through the capillaries is different from the cause which regulates the motion of the blood in the large vessels.

Dr. Poiseuille has endeavoured to examine with great attention the causes of the motion of the blood in those parts of vessels which have been isolated from the action of the heart by means of ligatures, or separated from the body by cutting instruments, and then to study the influence of the heart and arteries upon the capillary circulation.

He has established, by a great number of experiments, that the calibre which the arteries and veins present, proceeds from the pressure of the liquid which they contain; that their coats are constantly distended by the blood which they receive; that these vessels tend to collapse suddenly, in consequence of the elasticity of their coats, as soon as the cause of their dilatation is removed.

The large arteries and veins, as well as the small ones, possess this property; but, besides, the diameter of the last gradually diminishes when they cease to receive blood. This retraction is sometimes so great, that the mesenteric vessels of the frog, salamander, young rats and mice, are reduced to two-thirds of their original diameter. He has also ascertained that, *ceteris paribus*, this reaction is more decided in the arteries than in the veins. These facts being known, it is easy to determine the motions of the blood in parts which have been separated from the trunk either by ligature or by a cutting instrument—motions which even yet are designated by the title of circulation.

In fact, an attentive examination of this pretended circulation shows us, that the part being in a horizontal plane, the motion of the globules in the capillaries is totally at an end; that all the vessels, arteries, and veins, carry the blood from the extremities to the amputated surface; that this motion, becoming more and more slow, ceases after the expiration of some time, and at the same time the organ presents a much smaller quantity of blood. These motions result, then, simply from the approximation of the coats of the vessels towards their axis; they necessarily, therefore, drive the blood towards their open extremities. The tail of the frog, the foot of the same animal, the mesenteric of very young rats, of young mice, separated from the trunk by a cutting instrument, have presented constantly the same phenomena. This pressure which he has established with regard to the blood of animals, exists also in the liquids of vegetables; he also believes that this kind of circulation, which may be observed in the stipula of *Ficus elastica*, detached from the trunk, is due to the same cause.

The action of gravity, as well as that of heat, are also causes, but confined

within more narrow limits of the motion of the globules in parts separated from the trunk, especially when the blood has not yet coagulated in the vessels.

Numerous experiments made, 1st, upon the heads of the salamander and frog, animals in which the circulation is, as it were, suspended at pleasure, show that it is established gradually from the centre to the circumference; 2nd, upon the foot of the frog, dividing the crural vessels; 3d, upon the mesentery of the frog and salamander, by cutting the heart; 4th, upon the mesentery of young rats and mice. All these experiments, of which several are confirmed by those of the two celebrated physiologists, Haller and Spallanzani, have convinced Dr. Poiseuille that the heart and elasticity of the arterial coats are the *sole agents* in the capillary circulation in question.

In resting upon the preceding facts, that is to say, the action of the heart and arteries, and the tendency which the latter have to collapse when they are not sufficiently dilated by the tide of blood projected from the heart, the constant *jerk*, *intermittent*, and *oscillatory* circulation, which precede the death of an animal, are easily explained; the cause of the *retrograde circulation* presented by the arteries after the death of the animal and that of the heart is similar.

Having cleared up these points, the author passes on to the examination of the causes of the irregular motions of the globules which he has observed in the capillary vessels.

If we study the course of the blood in the arteries and veins of the frog, of very young rats and of young mice, we observe, in proceeding from the axis of the vessel to the coats, that the velocity of the globules is totally different. In the centre, their velocity is at a maximum; it diminishes gradually as we approach the coats. In the immediate neighbourhood of the coats, a very transparent space can be observed, which is generally occupied by serum; this space is equal to about the 1-8th or 1-10th of the diameter of the vessel. This transparent part of the vessels observed by Haller and Spallanzani as being occupied by serum, has been again noted by Blainville.

Since some of the globules, rubbing against each other, are projected into this transparent part of the vessels, the globules placed in the middle possess a very slow motion, and they cease to move when they are almost in contact with the coats of the vessel. The globules which are nearest to this transparent part have a double motion of rotation and translation; they roll, if the expression may be used, over this part of the serum.

From these observations, the author concludes that the interior of the vessels is lined with a layer of serum at rest. Since this layer is immovable in its immediate contact with the coats of the vessels, every time that a globule is placed there it will be at rest, or rather, its velocity will be more or less diminished, according to the portion of the globule immersed in it. Now, in the capillaries the globules move between two layers of serum. Hence, their motion ought to be less rapid than in the large vessels, since they require to overcome the inertia of this layer.

If a globule is partly in the layer, this portion of the globule will be at rest, while its remainder, placed in the axis of the vessel, will acquire a certain velocity; then the globule will move round its own axis, in order to acquire its normal velocity in following the centre of the vessel. If of two globules, one is placed in advance of the other in the layer, the former will pursue its course, and the latter will be delayed, and the motions described will be presented.

The labours of M. Girard upon the flow of liquids in tubes of small diameter have established, in most tubes susceptible of being softened by the liquid moving in them, the existence of a similar layer. The author passed through tubes of very small diameter, liquids holding in suspension opaque bodies; and, having examined this current by a microscope, he found this layer immovable, and of a thickness much smaller than that obtained by the calculations of Girard.

Hence, the author concludes, that the blood transported by the vessels of the heart to all parts of the body does not impinge against the coats; that a layer of serum, by its state of rest, guards the coats from any such effects. Besides, we can conceive the importance of this immovable layer of serum lining the coats of the vessels in the act of nutrition, since the recent experiments of Müller of Berlin have demonstrated, that the fibrin is held in solution by the serum.

Dr. Poiseuille has farther studied the influence of cold and heat upon this layer

of serum. The following experiment shows the result. At the temperature of  $77^{\circ}$ , he examined the circulation in the foot of a frog, and, in the vessel where the foot was placed, he deposited pieces of ice. In the large vessels, the transparent part of the serum obviously increased in thickness; the globules in immediate contact with it moved more slowly; the three orders of vessels, arteries, capillaries, and veins, preserved their diameters sensibly; the velocity in the capillaries was considerably diminished, and in some of these vessels it became insensible; during six or eight minutes, for example, the circulation in the capillaries of the other foot of the frog preserved its normal velocity: and it was not till a quarter of an hour after the submersion of the first foot in the iced water that the velocity of the blood in the second foot, placed in the atmosphere, was diminished, in consequence of the temperature of the whole mass of blood being sunk. The ice in the vessel was replaced by water at the temperature  $100^{\circ}$ , and the velocity of the globules became then so great, that their form could scarcely be distinguished. In young rats, the cold, applied only for a few minutes, stopped the circulation in the capillaries of the mesentery. It gradually resumed its powers, and acquired its normal rhythm after the ice was withdrawn.

Thus the diminished velocity of the capillary circulation by cold, and its greater rapidity by the action of cold, are naturally interpreted by the increase in the thickness of this layer in the first place, and its diminution in the second.

These results completely correspond with those of M. Girard on the variation in the thickness of the layer which lines the coats of inert tubes, when the temperature increases or diminishes.

We know that certain animals, such as fishes, and some amphibious mammalia, are sometimes immersed nearly  $262\frac{1}{2}$  feet (80 metres) beneath the surface of the water, and then support a pressure of from seven to eight atmospheres. It is important, therefore, to know how this layer acts, and at the same time to observe the modifications of the capillary circulation under such pressure. With this object in view, the author has constructed an apparatus, to which he has given the name of *Porte-objet pneumatique*. A short description will afford an idea of it, and develop the results which may be derived from its use. It consists of a strong box of copper; the top and bottom are formed of crystal, fitted into grooves placed in the sides. One of the extremities of this box carries a copper tube, which contains sometimes a barometer tube, and sometimes a manometer for compressed air; the other extremity presents a large opening by which the animals are introduced. To this extremity sometimes a suction pump is adapted, and sometimes a forcing pump. The animal, prepared in such a way as to allow the capillary circulation to be seen, is placed in the instrument, and the apparatus placed under the microscope. We can then observe the modifications which may introduce in the capillary circulation a more or less considerable pressure. In salamanders, frogs, tadpoles, young rats and mice, the arterial, venous, and capillary circulation have not presented any remarkable change, even when raising the pressure rapidly to 2, 3, 4, 6, and 8 atmospheres, and reciprocally. Farther, the circulation has continued to preserve the same rhythm even under a pressure of some centimètres (39 inches) in salamanders, frogs, and tadpoles. On placing in the apparatus very young rats and mice (it is well known that the mammalia, during the first days of their birth, may remain some hours without breathing) the circulation can be seen perfectly, *in vacuo*. How absurd, then, is the opinion of these philosophers who consider that, without atmospheric pressure, circulation cannot go on; but atmospheric pressure, combined with the motions of respiration, are accessory causes of the flow of the blood, as Dr. Poiseuille has shown in another memoir.

From these experiments, he infers, that the thickness of the layer of serum, the existence of which is due to the affinity subsisting between the coats of the vessels and the serum, a thickness which varies so remarkably from heat and cold, is independent of surrounding pressure, that the contractions of the heart preserve their normal rhythm, whatever the pressure is.

Several tubes of *chara*, placed in this apparatus, had presented, under a pressure, varying from 209 (7.8 inches) to 600 centimètres (23.6 inches) the same modes of circulation; and the motions of some infusorii contained in the water of the *chara*, such as *vorticelli*, *potifera*, &c., were executed with the same facility as under the influence of the atmosphere.—*Records of General Science from Bib. Univ. for November, 1835.*

4. *Observations and experiments upon the function of the Cæcum.*—By Dr. Schultz, Professor of Physiology in the University of Berlin. The objects of the learned Professor in making his experiments were to ascertain; 1, the function of the Cæcum; 2, the digestibility of the different articles of food; 3, the manner of the dissolution of the fleshy fibres in the stomach, according to microscopical observations; 4, the degree of acidity in the stomach and cæcum; 5, the degree of alkaliescence of the food in the stomachs of ruminating animals; 6, the nature of the acids in the stomach; 7, the coagulation of milk by the saliva, stomach, &c.: 8, the saliva; 9, the nature of the bile.

He maintains that there are two digestions, one in the stomach, the other in the cæcum, and that the latter is more especially active when vegetable food has been ingested. From the first experiment he learned that the degree of acidity in the cæcum is not always the same, that it is not always present, and that the food may even become alkaliescent. From the second, that this acidity was neutralized by long fasting, and thus allowed pure bile to enter the cæcum and neutralize its contents. From the third, that there is always bile in the course of the small intestines. From the fourth and fifth, that all the bile secreted by the liver during fasting, is by no means contained in the gall bladder, and that that part is very small compared with the large quantity that flows into the intestine during the empty state of the stomach. From the sixth, that though bile is always flowing, it never passes the cæcal valve during fasting, but collects on the upper side of it; it is only after perfect acidification, and at the beginning of the peristaltic motion of the intestines, that this bile flows into the cæcum. From the seventh, eighth, and ninth, that the degree of acidity and alkaliescence of various parts of the digestive canal, vary with the length of time that has passed after feeding, and the degree of perfection of the gastric digestion, as also with the length of time which animals have fasted before feeding. From the tenth, that the quantity of digestible matter which is contained in the food has a great influence upon the degree of acidity in the cæcum. From the eleventh, that in carnivorous animals, when the cæcum and colon are but little developed, the food is for the most part digested by the stomach and small intestines, and the acidity in the cæcum is in general very weak, since the food, when it is here collected, contains little or no digestible matter.

The general results from the whole of the experiments we give in the author's own words; they are of the highest interest and importance.

*Results of the experiments upon the cæcal digestion.*—It may, therefore, be gathered from my observations and experiments, that the food in the cæcum becomes not only a second time sour, but that the acid chyme is there neutralized by the access of bile, in the same way as in the duodenum; so that after the employing of the intestines very different reactions may be produced according to pleasure. On account of this twofold consumption of bile in the stomach and cæcum, there is an antagonism between the two digestions; for when the bile is consumed by the digestion in the stomach, the cæcal digestion cannot be perfected, and, on the other hand, when the bile flows into the cæcum, the neutralization of the acidity in the duodenum cannot take place. In those animals in which the cæcal digestion is most perfectly developed, this antagonism appears to be so arranged, that each digestion has its particular period of action, so that when the one is in action, the other is either lessened or at rest. In ruminating animals, it is very evident that the gastric digestion takes place more particularly during the day, and the cæcal at night, so that I think the gastric may very properly be called the diurnal, and the cæcal, nocturnal digestion.

In carnivorous animals, however, the cæcum is so little developed, that the stomach alone furnishes nearly the whole process of digestion. These animals, therefore, have a preponderating diurnal digestion. This agrees with the fact, that carnivorous animals rest for the most part during the day, and at night become hungry, and seek their prey, and are, therefore, nocturnal animals, since their digestion takes place during the day.

As the formation of the feces follows the perfected cæcal digestion, herbivorous animals are accustomed to discharge the greatest quantities in the morning and evening, and but very little during the day, and the healthy course of digestion. There appears to be something similar to this in man, in those ages where the cæcal digestion is most developed; in childhood, on the contrary, when the

digestive apparatus resembles that of carnivorous animals, repeated discharges of excrement take place at indefinite periods of time.

*"The use of the valvula cæci in cæcal digestion."*—That the cæcal digestion may take place, it is necessary that the still digestible remains of the food should be rendered acid and changed into chyme, as in the stomach, before its mixture with the bile. This could not happen if the bile flowed continually into the cæcum, and it is therefore probable, that its opening into the small intestines is closed during chymification, as the stomach is closed during its digestion, only with the difference which the different state of the matter required. The stomach is closed during digestion at the pyloric orifice, to prevent the egress of the food, and the cæcum at its iliac opening, to prevent the ingress of the bile. This is my view of the use of the valvula cæci. I have not only found in general at the lower end of the *ileum*, an alkaline reaction, while the upper is still either sour or neutral, but at certain periods of digestion, a collection of pure bile at the iliac orifice of the cæcum. The contents of the cæcum are at this time nevertheless sour. This would be impossible if the mouth of the cæcum were not closed during chymification. After the collection of the food, therefore, in the cæcum, its opening, like that of the bladder, uterus and stomach, appears to be strongly contracted by its muscular fibres, and with the help of the valvula cæci, to be perfectly closed. The contrary is the case at the beginning of the peristaltic motion of the cæcum, and upon the opening of its iliac orifice the collected bile flows in. This agrees with the sensation of the ceasing of the peristaltic motion after the collection of the food in the cæcum, which I observed upon myself, and have described in my work (*de Alimentorum Concoctione Experimenta Nova.*) It appears to me, therefore, that the generally admitted explanation of Fallopius, according to which, the use of the valvula cæci is to prevent the return of the food from the cæcum into the *ileum*, is quite unfounded; for it may be easily seen that during excretion this backward motion is very possible.

*"Concerning the times for eating corresponding to the periods of digestion."*—The simple rule to eat as often as one is hungry, appears no doubt the most natural. I shall be able, however, to show that this rule is by no means universal, and in many instances even pernicious. I shall succeed best if I prove that one is often hungry without having the least real need of food, and that this hunger is better allayed by fasting than by eating.

"Hunger is the feeling of need of the nourishing parts of the blood, and is situated in that organ through which it is satisfied—the stomach. This appears to be the reason, why, when the stomach is empty, we hunger, and not when it is full, even when it is filled with perfectly indigestible matter, which cannot satisfy the true feeling of hunger. We cannot judge from hunger whether the food be digested, and the proper source of hunger satisfied; and, therefore, this feeling cannot be the only rule for eating, since a perfect and undisturbed digestion is necessary for the true allaying of hunger, and, therefore, when digestion would be disturbed thereby, it would be improper to eat, notwithstanding hunger may be felt.

"If we consider under this point of view the antagonism between the gastric and cæcal digestion, it follows, that, if both be excited at the same time, they will reciprocally disturb each other, and that for perfect digestion, and the proper formation of the blood, they must take place at different periods of time. My experiments show that, for the completion of the cæcal digestion, the bile flows through the small intestine into the cæcum, and the whole intestine becomes thereby more or less alkaline; but they also show, that if, during the cæcal digestion, the stomach be put in action, the flowing of the bile to the cæcum is stopped, and its digestion consequently hindered, since the chyme cannot be neutralized, and all the contents of the canal as far as the excrement become sour. The formation of the blood in the lower parts of the canal is hereby disturbed, and the gastric digestion is also rendered imperfect; so that the two digestions cannot take place in their integrity at one and the same time. The completion, therefore, is indispensably necessary to perfect digestion in general, and it becomes very important so to regulate the periods for eating, that the activity of the stomach may not disturb the digestion in the cæcum.

"If now, the cæcal digestion be considered more particularly as a nocturnal digestion, the meals should be so regulated, that, when this begins, the stomach

may no more be put into action, or at least not overfilled. It is generally admitted that a man digests a moderate meal within three or four hours. My experiments, however, upon carnivorous animals, which digest much more quickly than the omnivorous, to which class man belongs, show, that six or seven hours are requisite for the digestion of a moderate meal, and that when dogs are allowed to eat as much meat as they will, twelve to fourteen hours are hardly sufficient for perfect digestion. The animals must even then be allowed to rest, for if they are made to exert themselves, the digestion is still further delayed, at least half the time longer. The observations which I made upon myself during intermittent fever agree with this fact, and it may safely be assumed, that six hours are necessary for perfect digestion, that is, till all sour chyme has disappeared from the stomach. If now it be admitted, that, in the common mode of life, the nocturnal digestion begins about from seven to eight o'clock in the evening, the last meal ought to be taken so early, that by this time it may be for the most part digested in the stomach. In general, therefore, one should not eat after four o'clock in the afternoon, and evening meals should be altogether avoided: for, in proportion to the lateness of the hour and the quantity eaten in the evening, the more will the cæcal, and therefore also the gastric digestion, be disturbed, seeing that both must take place nearly at the same time.

"That this rule is not so applicable in youth (as long as the gastric digestion resembles that of carnivorous animals, and for the most part finishes the process of digestion alone,) as in riper years, follows from what has been already observed.

"The more the digestion is disturbed by continued large and late evening meals, the less perfect will be the preparation of the blood, and the more will the need of its nourishing parts, or hunger, be felt, particularly in the evening when the stomach is empty. This is the reason why those people who should eat least in the evening are most hungry at this time, and here more particularly does the feeling of hunger not correspond with the period of digestion; for the more such people eat in the evening, the more imperfect is their sanguification, and the greater the consequent hunger, since the food goes almost entirely unchanged through the alimentary canal, and the nourishment it contains is lost to the body. The only means, therefore, by which this hunger may be properly appeased, is that by which the digestion may be restored, and sanguification amended, and that is, notwithstanding hunger, to abstain from food in the evening, that the gastric and cæcal digestions may not reciprocally disturb each other.

"The source of hunger will ever increase, by continually appeasing it by late evening meals, and in this way, therefore, it is as easy to starve from too much eating as from fasting: and no doubt in this way has many a person eaten himself to death, and most probably will still. We find in general that the meagerest and most sickly persons are the greatest eaters, and, on the contrary, the well-nourished and powerful eat less. That in the different condition of the digestive organs of different people, and according to the quantity of food which is taken during the day, there may be various changes and exceptions to this general rule, is self-evident. Where the youthful condition of the digestive organs is still vigorous, and the cæcum not as yet much developed, it is not necessary to be so particular about the smallness of the evening meals; and where there is in general but little taken during the day, and the quantity of food in the cæcum, therefore, small, more may be eaten in the evening, as there still remains a quantity of bile sufficient for the perfection of the cæcal digestion. The longer the fasting after meals, the more bile can flow into the cæcum after the ending of the gastric digestion, and the less fear need there be of evil consequences."—*London Medical and Surgical Journal*, October 31, 1835.

5. *On the Free-Martin*.—When a cow brings forth two calves, one a bull calf, the other to appearance a cow; the cow-calf is usually unfit for propagation and is called a *free-martin*. John Hunter has published some interesting observations on this subject, and given an account of the examination of three animals of this description. (Observations on the animal economy.) Dr. ALLNATT has recently examined a free-martin and gives the following as the result of his observations:—

"*Vagina* impervious, terminating in a cul de sac, which was curiously elongated at the extremity, connecting it with the uterus.

*Uterus* exceedingly small; a mere blind vermiform continuation of the vagina.

*Vesicula seminales* impervious; the extremities exhibiting a peculiar epididymoid structure.

Distinct traces of impervious Fallopian tubes, which were lost immediately above the fundus uteri.

*Vasa deferentia* pervious; but, instead of traversing to form the epididymis, terminating in a small prostate.

*Meatus urinarius* opened by a small curiously formed orifice laterally, and not by a direct continuation with the sulcus urinarius.

In connexion with the last mentioned fact, it was observed that the animal, during life, contorted its body into a most extraordinary emprosthotonic curvature, during the time of voiding its urine, and seemed perfectly incapable of performing the operation in its natural horizontal position."

The assertion of Mr. Hunter, that the free-martin *never* exhibits sexual propensities, is contradicted by Dr. Allnatt.

"A clergyman of great respectability," says Mr. A., "with whom I lately conversed upon the subject, informed me that he had bred a free-martin upon his estate, which had not only shown the natural desire for the male, but had actually admitted him—of course ineffectually; and I saw but yesterday, working in harness, a true, apparently female, adult free-martin, belonging to William Toovey, Esq. of Newnham, Oxfordshire, which occasionally manifests its male propensities in a most intelligible way. This gentleman informed me, that when the cows, amongst which this martin is kept, exhibit their natural fitness for the male, this creature, unlike the spayed heifer, or common ox, is at that period peculiarly on the alert, and has been observed to leap the cows in perfect resemblance of the entire bull.\* A gentleman also of this neighbourhood had a true free-martin killed, a short time since, which had taken the bull several times, but had never propagated: after death, scarcely a vestige of the uterus could be discovered."—*London Med. Gazette*, July 2, 1836.

6. *Spontaneity of movement in the fluids of living bodies.*—Professor Wm. P. ALISON has been for some time past endeavoring to form an estimate of the degree of evidence which may be adduced in favor of the physiological principle, to which some of the German physiologists give the name of spontaneity of movement; i. e. the evidence in favour of the proposition, that there exists in living bodies a principle of movement affecting their fluids, or the fluids in contact with them, which is peculiar to their living state, but independent of the contractions of any of their living solids, and which he has designated as consisting in vital attractions and repulsions.

It is a matter of essential importance to medical science, to have this point fairly decided; because if such principles of motion exist in living bodies, its alterations must certainly be the fundamental change in the production of the most frequent and formidable diseases. If such a principle exist, it must be in the capillary vessels of the higher animals, that it must chiefly operate; and it is generally agreed at the present day, that it is there that the most important diseases, whether general or local, originate; the disorders of the heart, and of the larger organs of circulation, which occur in disease, being generally subsequent to, and apparently consequent upon, changes in the state of the circulation in the small capillaries, either general or local.

Dr. A. thinks that a great variety of evidence may be fairly stated in proof of the existence of the principle in question, although there is certainly still room for careful scrutiny of the facts that have been thought to indicate it. Two branches of the inquiry Dr. A. considers as particularly deserving of attention. 1st, Whether the phenomena of inflammation can be explained, on the supposition that the only strictly vital powers moving the blood are the contractions of

\*This animal, in voiding its urine, strangely contorted its body, and was incapable of maintaining a continuous uninterrupted stream; but the fluid was emitted in a divided current at intervals, *per saltum*, and in small quantities at each jet.

the heart and blood-vessels, and other living solids; and 2nd, Whether the phenomena of respiration, and particularly the leading fact of the acceleration of the movement of the blood through the lungs, which is caused by exposure to oxygen there, can be explained on that supposition. Dr. A. thinks that the result of his experiments, taken in connexion with the facts already known, are nearly sufficient to entitle us to answer the question in the negative.

We have in a preceding No., (XXXV. p. 201) given a notice of the experiments of Dr. A. on the subjects just referred to, taken from the reports of the proceedings of the British Association; but as Dr. A. complains that these reports contain inaccuracies seriously affecting his meaning, we shall give the account he has furnished of them to our respected cotemporary, the *Edinburg Medical and Surgical Journal*, (January, 1836.)

7. *Additional observations on the vital properties of Arteries leading to inflamed parts.* By W. P. ALISON, M. D.—It may be remembered that the uniform result of the observations on this subject which I had the honour to communicate to the Association last year\* was, that the vital power of tonic contraction in arteries, to which Dr. Parry gave the name of tonicity,—which acts in all cases when a living artery is emptied of blood,—which determines the degree of contraction observed in arteries for some hours after death,—and which is the only vital power, ascertained by unequivocal experiment to exist in the arterial coats,—is found to be *diminished*, in all the vessels leading to an inflamed part, of a size admitting of actual measurement; and, taking these observations in connexion with those made with the microscope on the minute vessels of inflamed parts themselves, it may be laid down as a general fact, that all the vessels concerned in any local inflammation, are in a state of *relaxation and distension*, as compared with their natural condition.

It is correctly stated in the Report of the Transactions of this section, that a subsequent experiment was made by Dr. Fletcher, Mr. Dick and myself, in a different mode, but leading to the same conclusion. That experiment was made on portions of the axillary arteries of a horse, in which inflammation had existed for three weeks in one of the fore-legs; the violence of the original inflammation, produced by an injury, had abated; but a blister had been applied three days before death, by which fresh inflammation had been excited. Bent tubes and stop-cocks were used in the manner practised by Poisseulle to try the contractile power of the vessels.† Two portions of the axillary arteries, each eleven lines long, were successively fastened between two bent tubes, and distended with water, by the pressure of a column of mercury of about ten inches, introduced into one tube, and then allowed to react on the water distending them, so as to raise the level of a column of water, in the other tube, to a degree which could be accurately measured. The result was, that the reaction of the artery of the sound limb raised its level by sixteen lines, and that of the artery of the inflamed limb raised it only by ten lines. But as this experiment was made on an animal already dead twelve hours, it might reasonably be said, that it afforded a measure of the elasticity only, not of the peculiar vital power, of the arteries examined.

In a subsequent experiment made on the 28th July last, in which I had not only the able assistance of Mr. Dick, but likewise of Dr. Allen Thomson, Dr. J. Reid, and Dr. Spital, a horse was bled to death, on account of a violent inflammation, attended with much pain and swelling, of eight days standing, in the knee-joint of one of his fore legs; and portions of the two axillary and humeral arteries, precisely corresponding to one another, and about  $2\frac{1}{2}$  inches in length, were examined as soon as they could be taken from the body. The artery of the diseased limb, when taken out of the body, in this as in all the other cases, was decidedly the largest, measuring at the larger extremity of the portion taken, 12·8 of an inch in circumference, and at the smaller  $\frac{1}{2}$ ; while the circumference of the artery of the sound limb at the corresponding points was 11·8 and  $\frac{1}{2}$  only. The trial of the strength of the arteries by their reaction on water distending them was made in two ways;—*First*, the second limb of the tube was filled up with water, and the space occupied by the quantity expelled on the contraction of the artery was

\* See fourth Report of British Association, p. 764, and Vol. XVII. p. 183 of this Journal.

† See Magendie's *Journal de Physiologie*, tome viii.

measured. The artery of the sound limb was found to expel just .5 inch of water; that of the diseased limb .35 inch only. *Secondly*, what appeared to be the more accurate mode, the water was made to fill the second tube only to a given point, and the rise of its level, on the expulsion of that contained in the distended portion of artery, was accurately measured, and amounted to 1.75 inch in the case of the artery of the sound limb; and to 1.25 inch only in the case of that of the inflamed limb. The column of mercury used to effect the distension was exactly the same in all the trials.

Another observation was made with much care and accuracy, by Dr. Spittal, on the relative circumference of these portions of arteries at this time, and again after a lapse of about twenty-eight hours, when the *post mortem* contractions dependent on the last exertions of their tonicity were over. The artery of the sound limb was then found to have dilated itself again from 33-24, to 41-24;—while the artery of the inflamed limb had dilated itself only from 36-24 to 42-24, at the corresponding point; so that the difference between the artery in its contracted state immediately on death, and in its dilated and perfectly dead state was 8-24 or  $\frac{1}{3}$  in the sound limb, and only 6-24 or  $\frac{1}{4}$  in the inflamed limb; and in other parts of the arteries it was obvious, that the difference of calibre between the vessel in its contracted state immediately after death, and its subsequently dilated state, (which is the measure of the last exertion of its tonic power of contraction,) was considerably less in the inflamed than in the sound limb; the vessels approaching more to equality of calibre at the second examination.

It appears, therefore, as the uniform result of all the modes of examination which we have been able to devise, that the arteries of inflamed parts, examined at different parts of their course, and at various periods of the inflammation, and when they are pulsating apparently with unusual force, are in fact *relaxed* or *weakened* arteries; transmitting, in consequence of their weakened condition, the impulse given by the heart, with less modification than it receives in passing through the arteries of sound parts; but themselves exerting a less power of contraction on the blood, than they do in the sound state.

This being ascertained, the question immediately presents itself, whether this weakened state of the vessels is an adequate cause for all the changes, as to the movement of the blood, which take place in the inflamed state, *i. e.* whether inflammation consists simply in a weakened action of vessels. I formerly mentioned one striking fact, on which I think there can be no doubt, as sufficient to indicate that this question must be answered in the negative,—viz. that two distinct and nearly opposite changes are ascertained to take place in the movement of the blood through an inflamed part,—a retarded movement in the vessels most immediately concerned, and an accelerated movement and greatly increased transmission, in all the surrounding vessels.

I shall take the liberty of adding one or two other considerations, in confirmation of the opinion, that this diminution of the contractile power of the arteries, although the only change perceptible in their mode of action, is quite inadequate to explain the simplest and most essential phenomena of inflammation.

1. If we suppose inflammation to consist only in an altered action of vessels, and this alteration to be of the kind which these observations indicate, we make no attempt to explain the difference between inflammation, with effusion of lymph, and simple determination or congestion of blood, where no unusual product issues from the vessels. Now this appearance of the peculiar *products* of inflammation, exuding from the vessels, is what the pathologist regards as most truly characteristic of the inflamed state; and I believe it to be certain, that in the usual effusions, consequent on healthy inflammation, the fibrin is both in much larger quantity, and shows a *greater attraction of aggregation*, than in any effusions from uninfamed vessels. Of this we have a familiar example in the case of the lungs, which we often find not only infiltrated with effused serum, but so far condensed by congestion of blood, as to be impervious to air, and not crepitate when cut, without being so far increased in density as to sink in water; but so far as I have observed, whenever we see the granulations of decolorized lymph in the air cells, truly characteristic of inflammatory effusion, then we are certain of seeing this decisive proof of the increased density of the lungs.

2. If we suppose inflammation to consist merely in that relaxed condition of the vessels which is indicated by these observations, we must suppose the usual

causes of inflammation, *e. g.* mechanical injury, to produce, very rapidly, in all the vessels adjoining and approaching to the part to which they are applied, this state of relaxation or weakness, and to be effectual in exciting inflammation, only inasmuch as they diminish the vital power of those vessels. Now this depressing or sedative effect on vital power is not only just the reverse of the effect produced by such agents on other living parts, whether irritable or sensible, but is just the reverse of the effect produced by such agents on arteries themselves, when they are applied in such a manner as directly and exclusively to irritate their coats; as in the experiments of Verschuier, Dr. Thomson, Dr. Hastings, Wedemeyer, and others, where the contractile power of arteries was decidedly excited by irritation, their calibre in consequence *lessened*, and the transmission of blood through them *impeded*.

The more that this subject is considered, the more distinctly, as I think, must it be perceived, that the only way to escape from these various difficulties is to suppose, that the causes which excite inflammation do so by really exciting or increasing a vital action, but not an action of the nature of contraction; that the idea of the vessels of inflamed parts taking on an increased action is a delusion; that what is truly excited is an action *not of the vessels but within the vessels* of the part affected; that it is an increased exertion of powers by which the blood is moved, or its motion influenced, in the capillaries, but which powers are inherent in the blood itself, dependent on, or influenced by, its relation to the surrounding textures, but independent of any contractions of living solids; and the increased exertion of which powers leads always to distension, and to more or less diminution of the tonic power of contraction, of the vessels, within which it takes place.

8. *Experiments illustrating the immediate cause of death in Asphyxia.*—By W. P. ALISON, M. D.—The experiments on this subject which I am to relate are so simple, and their result so consonant to former observations, that in order to show them to be in any degree worthy of the attention of the Association, I must enter on a short explanation of the point which they are designed to illustrate.

I take it to be well ascertained, by the experiments of Dr. Williams of Liverpool, and Dr. Kay of Manchester, that in ordinary cases of Asphyxia, the circulation of the blood is brought to a stand, not in consequence of the venous blood penetrating either the nervous or muscular textures of the body, but in consequence of its gradually failing to penetrate the lungs, and not being delivered to the left side of the heart in sufficient quantity to maintain effective actions there.

It appears, therefore, that the admission of air into the lungs in respiration is, in some way or other, a powerful auxiliary cause of the circulation through these organs, and that when that auxiliary power is withdrawn, the contractions of the right side of the heart, although continuing vigorous for some time, soon become ineffectual in propelling the blood through the lungs.

The question immediately presents itself, in what manner do the actions of respiration afford this auxiliary power to the circulation through the lungs?

It may be remembered, that the doctrine of Haller on this subject, prior to the speculations either of Goodwyn or Bichat, was, that the actions of respiration are a merely mechanical auxiliary to the circulation, and that asphyxia depends simply on the blood stagnating in the lungs, in consequence of the suspension of the natural alternate contractions and expansions of their substance. After the theories both of Goodwyn and Bichat had been shown to be erroneous, it is not surprising that this idea should be revived, and stated in explanation of the death by asphyxia; as has been done in an ingenious article in the *Edinburgh Medical and Surgical Journal*.

The obvious method of bringing this theory to the test of experiment is to ascertain, whether the gradual stagnation of the blood in the lungs, and in the right side of the heart, takes place equally in an animal asphyxiated by breathing a gas which contains no oxygen—where the mechanical actions of respiration go on even with increased force, although the chemical changes are suspended—as in one where the mechanical part of the process is suppressed by hanging or drowning.

It is stated by several physiologists, and particularly by Mr. Broughton, that in animals asphyxiated by breathing azote, the right side of the heart is found distended with blood, and contractile, and the left side nearly empty and motionless,

as in other cases of asphyxia. But to this observation it may be objected, that the animals were allowed to lie in the azote until they became insensible, and it was only some minutes thereafter that their chests were opened; and that as the actions of respiration would necessarily become slow and imperfect, and then cease entirely (whether from the presence of venous blood or the deficiency of arterial in the brain, we need not inquire,) some time before the examination was made, the impediment to the circulation, supposed to be consequent on the suspension of the mechanical acts of respiration, would have time to show itself.

I therefore thought that, in order to decide this point satisfactorily, it would be necessary to have an animal confined in azote, not until it became insensible, but only until its respiration became laboured; and then taken out and immediately killed, in the manner most likely to arrest its circulation instantaneously, so as to give us an opportunity of judging, whether the blood in this case fails to make its way through the capillaries of the lungs, even while the mechanical actions of respiration are going on with full force.

With the assistance of my friend, Dr. J. Reid, I therefore performed the following experiment repeatedly.

A rabbit was confined under a bell glass in azote, until its breathing became obviously laboured, not more frequent than usual, (in fact, generally less frequent, the respiration of that animal, especially when it is at all frightened, being very rapid,) but distinctly fuller, longer, and more forcible. The bell glass was then removed as quickly as possible, and the animal instantly struck on the head, with such force as to crush the brain and *medulla oblongata* completely. It never appeared to inspire after that injury, but was always strongly and generally convulsed. As soon as the convulsion was over, the chest was opened, and the state of the heart and great vessels examined. We were rather surprised to find, that, notwithstanding the extent and violence of the injury of the brain, the right side of the heart was never completely stilled, although its contractions were feeble and quivering, and obviously ineffectual for maintaining the circulation. The movement of the blood towards the right side of the heart by the great veins likewise always continued to a certain degree; but in these respects the appearances were the same as in comparative trials on animals in the natural state.

What was essentially important was the observation made in all the animals which had been confined in azote, and also in one which had been confined in a small quantity of air, until its breathing had become hurried and laboured, viz. That the right side of the heart was always found, from the time it could be brought into view, distended with blood, and the left side comparatively empty. The quantity of blood got by puncturing the pulmonary artery and pressing the right side of the heart, was from five to ten times as much as could be obtained in the same way from the left side—a difference greater than is sometimes observed in an animal that has been hanged. The right side of the heart, as already stated, was always found pulsating feebly, though not maintaining the circulation. The left side of the heart, evidently in consequence of defect of blood in it, was always found motionless; whereas, in one of the observations on an animal breathing naturally, until it was killed in this way, this side was found pulsating, although feebly, for a short time.

I think it evident, from these experiments, that the explanation of the failure of the circulation in Asphyxia, by the suspension of the mechanical process of expansion and contraction of the lungs is erroneous; that the circulation fails equally, and is obstructed at the same spot, although the movements of respiration continue, if oxygen be not inspired; and that *air*, or a gas containing oxygen, is possessed of a peculiar efficacy in promoting the flow of blood through the lungs; and the question immediately recurs on us—in what manner does the air, admitted into contact with the blood at the lungs, promote its flow through their capillaries?

If we suppose the air to effect this purpose by acting as a stimulus to the capillaries of the lungs, we lay ourselves open to two objections which seem quite insurmountable. 1. We suppose the minutest capillaries of the lungs (certainly the vessels which are immediately exposed to the air) to be vessels capable of contraction; whereas the most careful observations on the blood moving on the sides of the air cells of the lungs, leave it very doubtful whether it is there confined within vessels at all, and certainly give no countenance to the supposition,

that the smallest channels or tracks in which it moves are susceptible of vital contraction.\* 2. We suppose the effect of the stimulus of the air, acting on these vessels, to be the very reverse of what has been observed in every case where either this or any other stimulus has decidedly acted on a living artery; because in all such cases, and particularly in the experiments of Wedemeyer, where stimulating liquids were introduced into living arteries, and obviously caused constriction of their smaller branches, the effect was, not to promote, but to retard and impede the transmission of their contents.

If again, we suppose the effect of air on the minute arteries in the lungs to be, to cause relaxation or distension, *i. e.* to act as a sedative on them, we attribute to the air, in its action on these vessels, an effect the very reverse of that which is observed, when any other vessel is exposed to that agent; the change observed in such cases (when any change is observed) being always of the nature of tonic contraction.

The conclusion, therefore, appears to me to be nearly inevitable, that the application of air to the blood in the lungs promotes the flow of the blood there, in a manner wholly inexplicable, either by the mechanical process by which the air is applied, or by any influence which the air can exert, on the contractile power of the vessels through which the blood moves. And if this be so, I think the reasonable inference must be, that the motion of the blood through the lungs, in a living and breathing animal, is partly determined by causes which are independent of any impulse, or modification of impulse, by the solids containing the blood.

This inference will be strongly supported, if it shall appear, on farther examination, to be the fact, as seems to me nearly ascertained, that in some of the lower classes of animals and in vegetables, the mutual action of the living fluids and of the air is provided for, by movements of fluids, but independently of any muscular contractions of solids, in the respiratory organs. And I cannot help indulging so far in conjecture as to anticipate, that the important discovery of Purkinje, of the currents visible in the air passages of animals, will be found to support the same inference. For although these currents have been ascribed, both by the German and English physiologists who have observed them, to the vibratory motion of minute *cilia*, yet I am strongly inclined, from what I have myself observed of such currents in several other instances to believe, that the movement of the *cilia*, although frequently an accompaniment, and perhaps an auxiliary, is not an adequate cause of them; and that their real cause is more accurately expressed by the terms used by a recent French author, who ascribes them to a *jeu d'attraction et repulsion* between the living fluids and the air.

## PATHOLOGY.

9. *On Certain Collections in the Alimentary Canal.*—It is a remarkable, though a well established fact, that solid matters sometimes become impacted in the colon, and retain their situation, even after long courses of purgative medicines; the watery evacuations passing over without dislodging the solid cake. Cases of this description often occur where they are not suspected, and they are frequently mistaken for disease of the liver, and maltreated accordingly. The following extract from a paper, by Dr. JAMES MURRAY, of Dublin, contained in a recent No. (July 30, 1836) of the *Lancet*, presents some interesting examples of such collections.

"1. *Magnesia as an Intestinal Concretion.*—The first case of magnesian concretion on public record, occurred to me in 1812. A child became much distressed by tormina and tenesmus, fulness and tenderness of the abdomen, vomiting, colic pains, frequent watery evacuations, spasms, and nervous twitchings, with febrile exacerbations. Symptoms resembling those of hydrocephalus began to exhibit themselves; Mr. McCluney, a respectable surgeon of Belfast, was in attendance with me. After the administration of castor oil, rhubarb, and the hydrarg. cum creta, the child began to pass off large quantities of hard alvine concretions, like peas. Having submitted them to analysis, I found that they consisted of magnesia, enveloped in the mucus of the canal. By the continued use of mild ape-

\* See Marshall Hall on the Circulation, pp. 40, 47, and Pl. viii.